School Choice, Incentives, and Academic Outcomes: Evidence for Chile*

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February, 2004

Abstract

This paper examines the effects of inter-school competition on student outcomes by using exogenous variation in the availability of private schools in Chile. Given that naïve estimates of the effects of competition on student outcomes are biased by endogenous entry of schools, this paper uses variation in the number of Catholic priests per capita in different school markets, as an exogenous determinant of the supply of private schools. Results suggest that greater competition significantly raises both test scores and the productivity of schools. There is also evidence that the effects of school choice are significantly larger for students attending subsidized private schools, and insignificant or even negative for students attending public schools facing softer budget constraints.

* I would like to thank the Ministry of Education of Chile for providing most of the data used in this paper, especially Mauricio Jélvez and Claudia Matus.

1. Introduction

Extending school choice has been one of the most debated topics in recent years in the public arena and in the Economics profession (for references, see Hoxby, 2003; Ladd, 2002; and, Neal, 2002). Regarding analytical issues, almost everything has been proposed. On one extreme we have models suggesting significant productivity effects of more choice (creating competitive pressures on schools that might be behaving as local monopolies). On the other extreme, some papers stress markets imperfections and predict increased segregation without significant increases in school quality. Regarding empirical research, some papers analyze the effect of attending private vs. public schools using exogenous variation to identify random assignment to each school (e.g. Angrist et al., 2002). Other papers have focused on the effects of inter-school competition as a measure of school choice on productivity (e.g. Hoxby, 2000).

In this paper, I will follow and extend the second line of empirical research, studying the effect of inter-school competition on education outcomes in Chile using data for 2002. Chile is interesting because it is the only country having the whole K-12 sector operating under a "quasi-voucher" system for a long period of time (since 1981). This has created a sizeable publicly-subsidized private sector that serves roughly one third of students. Moreover, private enrollment varies significantly among different school markets (i.e. geographic areas). This source of variation will be used in this paper to measure the effects of choice on educational outcomes.

However, as discussed below, there are some conditions for a proper operation of a voucher scheme that has not been implemented in Chile so far. Namely, agents (teachers and administrators) operating publicly-owned schools do not have a clear link between school outcomes and their welfare (i.e. their income and employment), as assumed in a ideal voucher scheme. Moreover, most public schools seem to operate under a "soft budget constrain". That is why I called the system, a "quasi-voucher" scheme. Subsidized private schools are closer to an ideal voucher system, where school budgets and welfare of agents are related to enrollment and, therefore, they should react strongly to the incentives provided by vouchers. These differences will be exploited to analyze if the effects of choice depend on differences in incentives structures of schools.

Methodologically, the most important challenge is that naïve estimates of the effects of competition on student outcomes are biased by potentially endogenous entry of schools in areas having low school quality. To overcome this problem, I will extend previous work by Hoxby (1994), by using the number of Catholic priests per person by school market as an instrument for private school enrollment (my proposed measure of inter-school competition, see below), after controlling for the size of the Catholic population. The basic motivation is that a Catholic priest is linked with a parish, which in general has attached a school. This creates a source of exogenous supply of private schools in different areas, given that about 40% of Chilean private schools are Catholic. Both qualitative and quantitative evidence is provided in order to give some support for the identification strategy. Moreover, by controlling for the share of Catholic population I am taking in account direct effects of this variable on educational results (as suggested by an incipient research on the effects of religions on a number of variables, e.g. Barro and McCleary, 2003). Thus, this improves the identification strategy of Hoxby (1994), which uses religious affiliation in different areas as an instrument, and, therefore, may violate exclusion restrictions.

Another methodological challenge is distinguishing between changes in school behavior related to productivity and in student populations. So, following Hoxby (2000), I will control for (i) mean and standard deviation of mother education at the class, school, and market levels as proxies for student's characteristics, peer effects, and market effects, and (ii) interactions of the proposed measure of competition and heterogeneity of students characteristics.

The paper is organized as follows. Section 2 briefly describes the Chilean education sector and previous research. Section 3 presents a succinct theoretical motivation, the equations to be estimated and a detailed description of the identification strategy. Section 4 presents data, basic results, and some robustness checks. Section 5 briefly concludes.

2. Primary and secondary education in Chile

A simple way of describing the K-12 sector is by comparing its characteristics before and after the 1981 reform. Previous to the reform, the central government was

involved in the funding and production of education; supervision and regulation of curriculums; handling of human resources; and investment. The 1981 reform changes that situation. Public education was transferred from the central government to the municipalities (local governments); private and public schools started to receive a per-student subsidy (voucher) depending on enrollment; parents were free to choose among any publicly-financed school; and would-be schools were free to enter in the market.

Thus, publicly-funded education in Chile is organized in a mixed scheme, with private and public schools receiving transfers accordingly to established rules. That is the main difference with the pre-reform period when public financing to private schools was minimum and discretional.[1] Three types of schools emerged: Publicly owned schools (managed by local governments), subsidized private schools, and unsubsidized private schools. The first two kinds of schools receive the per-student subsidy. The latter does not receive public money, charges high tuitions and is focused in upper income students.

In addition, there are important differences between public and private schools in terms of incentive structures and external resources. Public schools work under "softer" budget constraints: when needed, public schools losing students receive transfers beyond and above the voucher to pay schools' expenses (Sapelli, 2003). Regarding teacher regulations, teachers in public schools can not be easily removed from their positions. In short, at least for some public schools, budgets and agents' incomes are *actually* fixed and do not depend on enrollment, creating what I would call the key missing incentive mechanism in the Chilean voucher system.

Another point related to the voucher scheme is that the per-student subsidy is almost invariable in student characteristics. As argued by Hoxby (2001), if the cost of educating students is decreasing in, say, mother education (as a proxy of "pre-school" student characteristics) and the public subsidy is flat, schools may compete for students with better backgrounds, driving away the rents created by them. Alike, parents would demand more intensely schools providing better peers. This point highlights the importance of controlling for peer-group effects in empirical analyses.

[1] Notice that a group of free private schools did receive public funding, but the amount of resources was small and discretional.

Although the main elements of the 1981 reform have remained constant, there are some relevant developments to be mentioned. First, while the per-student subvention dropped 55% *in real terms* from 1982 to 1990; it has increased by an average of 6% real each year from then on.[2] Second, there was little public information on school quality available for parents until the mid 1990s, afterwards test scores have become hugely available (from the late 1990s, they are available on the Ministry of Education webpage). Third, with local governments not democratically elected during the 1980s, there was little local pressure to improve public school quality. Fourth, from 1990 on, the government channeled additional resources to "vulnerable" schools, increased non-voucher spending, created rigidities in the teachers market, and allowed to some subsidized schools to charge co-payments. Finally, since the mid 1990s, the government started some programs assigning resources based on educational results.

In terms of empirical research analyzing the Chilean reform, a number of recent papers use datasets containing student information and show an advantage of subsidized private schools over public schools, after controlling for student characteristics (e.g. Contreras, 2002; Sapelli and Vial, 2002 and 2003). However, no paper uses a valid source of exogenous variation for the assignment of students to schools. Most research uses the availability of different schools as instruments. But, exclusion restrictions are violated if school choice (i.e. the availability of schools) has a direct effect on outcomes.

Other research has tried to identify effects of school choice (using private enrollment as a measure of inter-school competition) on student outcomes. Here results are controversial. On the one hand, Hsieh and Urquiola (2003) and McEwan and Carnoy (1999) find negligible effects of competition. Hsieh and Urquiola present regressions for municipality-level outcomes for the 1980s. McEwan and Carnoy (1999) present OLS estimates using a panel of schools with information from the 1980s to the mid 1990s. On the other hand, Gallego (2002) present IV-estimates using information at the school level for 1994-1997 and finds significant effects of competition.

Differences in results come from two facts. The first is the time period included in regressions. It is hard to argue that the 1980s was a period when some basic conditions of

[2] Notice that this is very important because the per-student subsidy was supposed to cover operational expenses and, therefore, the drop was probably associated to a significant decrease in the quality of subsidized schools.

the voucher system were implemented (i.e. the significant drop in subsidies, no information available, absence of local pressures). The second difference has to do with data aggregation. Hsieh and Urquiola use aggregate data because they claim that by doing so can better disentangle between selection and productivity effects.[3] Gallego (2002) use school data because he claims that there is a lot of intra-market variation in outcomes.[4] This difference matters because running aggregate regressions wastes a lot of information on individual and school level heterogeneity (Hoxby, 2000). But, the cost of analyzing individual effects is that peer-group and market effects have to be taken carefully in account. For doing so, individual information is crucial.

It remains a more fundamental identification problem in available estimates of the effect of competition for the Chilean case (Hoxby, forthcoming). Papers identify the exogenous variation in private enrollment by using differences in population sizes and in urbanization rates among geographic areas. Even though, over-identification tests are not rejected, it might be the case that (i) what we are observing is migration related to education, and therefore, population sizes would be endogenous to pre-reform school outcomes (Hoxby, forthcoming) and (ii) both variables can have direct effects on school quality, because they may be capturing idiosyncratic characteristics of cities that may have direct effects on education results.[5] Given the limitations of over-identification tests, some more credible identification strategy is needed. This paper will try to do so.

3. Proposed model and identification strategy

This section starts by briefly presenting the rationale for expecting (or not) effects of choice on school behavior and outcomes. The basic idea for expecting significant effects is an agency problem between schools (administrators and teachers) and parents (students). Along the lines of Manski (1992), let us assume that schools maximize rents (equal to the difference between total expenses and expenses valued by students). Parents pick the school that maximizes students' welfare (that depends on expenses valued by them). Introducing school choice (i.e. more options) lowers the cost of moving students among schools and/or releases information about the true productivity of schools (Hoxby,

[3] This claim depends on the particular functional form for peer-group effects that they use.
[4] For instance, the intra-cluster correlation at the school market level of 2002 test scores is only 0.065.
[5] Glaeser and Mare (2001) show that there seems to be greater skill accumulation in urban areas.

1994). In order to keep enrollment, this produces an increase in schools' expenses of resources valued by students, as long as their budgets depend on enrollment. If not, there are little incentives for them to change behavior (or, even, there may be perverse effects of losing students: having a fixed budget with fewer students increases schools' rents; Sapelli, 2003). Thus, the response of schools to choice depends on the interaction among their objectives, and their incentives structure (linking enrollment and budget).

There also exist arguments questioning the effects of choice on school quality, especially for students coming from poorer families (see Carnoy, 1997 and Hsieh and Urquiola, 2003 for details). Poor families would not care about educational quality received by their children. There would not be suitable information on schools quality. There would exist high costs of moving students and low mobility, which would create local monopolies, limiting the effect of choice.[6] Assuming (which is, in general, not explicit in most papers) that it is cheaper to improve quality by selecting better students than by increasing effort or efficiency, more inter-school competition could even produce negative effects on some students because of selection effects.

As theoretical arguments may go either way, I will empirically evaluate both points of view for the Chilean case by analyzing the relationship between a measure of school choice and academic outcomes, after controlling for socioeconomic-variables at the class, school, and market level. In terms of empirical specification, following Hoxby (1994 and 2000), the equation to be estimated is:

(1) $H_{icsm} = \pi C_m + \alpha X_{icsm} + \beta \hat{X}_{csm} + \sigma \hat{X}_{sm} + \kappa \hat{X}_m + \rho Y_m + \varpi_m + \varepsilon_{icsm}$,

for student *i* in class *c*, school *s*, and educational market *m*. *H* is some student outcome, *C* is a (potentially endogenous) measure of competition in market *m*, X is a measure of pre-school characteristics of students (e.g. mother education), $\hat{X}$ is a vector including the mean and standard deviation of *X* grouped by classroom, school, and market, *Y* are some exogenous variables, ϖ_m is unexplained schooling market outcome (see below for its rationale), and ε is a student-specific error term.[7] [8]

[6] However, Hoxby (1999) points out that what really is needed for creating incentives is that at least a fraction of students are on the move at any given time.

[7] The calculation of standard errors in (1) should reflect the fact that we are using multiple observations of variables in classrooms, schools, and markets that are not independent. This problem is solved by using

An initial empirical challenge when estimating (1) is how C_m is measured. First, a definition of school market is needed. In this paper, I make use of about 280 municipalities and the Greater Santiago as proxies for local educational markets. This choice is motivated by the fact that with the exception of municipalities in the Greater Santiago, the overwhelming mass of students attends schools in the town where they live (Gallego, 2002; Hsieh and Urquiola, 2003; and Sapelli and Vial, 2002). Second, I need a measure of school choice in each market.[9] To do that, I use the information provided by the share of private schools in total enrollment in a market (i.e. students enrolled in private schools over total enrollment). Considering that the number of public schools has remained roughly constant since the reform and that most entry in the post-reform period comes from private schools, variation in private enrollment quantifies availability of schools in different areas. Putting differently, a higher ratio of private enrollment identifies an area where more students are in the margin between different schools.[10]

A second, more fundamental, challenge is that C_m may not be exogenous. To makes things clear, let us assume that the reduced form of the determinants of the private enrollment share is:

(2) $C_m = \delta Z_m + \theta \hat{X}_m + Y_m + \phi \varpi_m + \nu_m$,

where Z is some exogenous determinant of C_m, and ν is a market-specific error term. The way the potential endogeneity of C_m is modeled is by assuming that the demand for private schooling is a decreasing function of ϖ_m because the demand for private schools should be larger in areas with bad pre-existent public schools (Hoxby, 1994). Thus, (1) and (2) imply that $\text{cov}(H_{icsm}, \varpi_m + \varepsilon_{icsm}) \neq 0$. Therefore, OLS estimates of π are in general inconsistent. The direction of the bias depends on ϕ, which is negative by assumption.

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corrected standard errors, as proposed by Moulton (1986). This correction allows each classroom, school, and market to have a random effect (which is not presented in (1) for shortness sake).

[8] In addition, as shown in Hoxby (2000), when estimating (1) using IV methods and including heterogeneity in each school as a dependent variable, it is necessary to include in equation (1) interactions of C_m and average school heterogeneity by market (using the interaction of Z_m and average heterogeneity by market as instrument). These terms encompass the way choice interacts with market heterogeneity to affect school heterogeneity.

[9] The fact that we are using an imperfect measure of competition is an additional motivation for using an IV strategy for estimating equation (1). Hoxby (2000) discusses cases when non-classical measurement error may produce OLS estimates that are not only biased, but also present the wrong sign.

[10] Thus, hereafter I mean school choice when I talk of inter-school competition, private enrollment, or private competition.

Next, I need some theory of how the degree of private enrollment varies among markets. I am looking for some exogenous source of variation in the share of private enrollment in different markets (i.e. Z_m in eq. 2). To do so, I use variation in priests per person among different areas, after controlling for the share of Catholic population. This variable attempts to capture exogenous differences in the supply of private schools. As previously mentioned, a significant share of private schools in Chile is Catholic and, from the past, the work of Catholic priests and missionaries has been strongly related to education.[11] The involvement of priests in education has been understood as a key element of their religious mission.[12] In general, priests founded parishes that had attached a school. Thus, most Catholic schools seem to share infrastructure, fixed costs, and personnel with other (mostly confessional) activities.

What is interesting about Chile is that, despite the majority of the population being Catholic (all other denominations represent less than 10%), there is substantial variation in the number of priests per people in different areas. While the average school market has a ratio of about 0.16 priests per 1,000 people, the market with the higher ratio (0.23 priests per 1,000 people) has more priests per person than most Latin American (and, overwhelmingly Catholic) countries; and the market having the lowest ratio (0.06 priests per 1,000 people) is comparable to what is observed in a poor (and non-Catholic country) such as Kenya.

This has to do with the fact that characteristics of Catholic dioceses seems to be highly specific to individual bishops and other church leaders and the prominence of the religion order that is managing the district. On the one hand, most Chilean dioceses were assigned to different orders when they were established. These groups have different numbers of priests available to be sent as workers. On the other hand, the number of religious vocations (and, therefore, priests) in an area seems to depend on particular aspects of the personality of the bishop. In addition, differences in priests per person have

[11] In a broader context than Chile, I would have also mentioned the role of missionaries of other religions in expanding education (see Woodberry, 2003). However, in Chile this does not apply because non-Catholic missionaries, and especially their presence in the education sector, are not very significant.

[12] For instance, in a document defining the role of a Catholic school, Garrone (1977) states that "The Church establishes her own schools because she considers them as a privileged means of promoting the formation of the whole man, since the school is a centre in which a specific concept of the world, of man, and of history is developed and conveyed".

historical roots; the correlation of priests per capita during the 1990s and the 1950s is 0.61 (for a sub-sample of dioceses having data in both periods).[13]

In order to study, at least indirectly, the validity of the identification strategy, two empirical exercises were conducted. First, Table 1 presents cross-country regressions showing that countries having more priests per capita have a larger share of private enrollment in schools, after controlling for a number of covariates (namely, denominational variables).[14] This result is a sort of international generalization of the assertion that, *controlling for Catholic affiliation*, the availability of priests is correlated with private school enrollment and, therefore, giving empirical support to the theory motivating the proposed instrument and putting it in a broader context than the Chile.

Second, Table 2 presents results coming from regressions showing that the ratio of priests per capita does not seem to be related in a significant way with other socioeconomic outcomes in Chilean municipalities, controlling for covariates of Z_m included in (2). Dependent variables are proxies for municipality-level variation in health conditions (infant mortality), level and variability of income, and security (violent crime).[15] This evidence gives some indirect support to our exclusion restriction (that Z_m has no direct effect on student outcomes above and beyond its effect on C_m), therefore ruling out the worry that priests per-capita might be capturing some omitted characteristic of area that may affect student outcomes.

4. Data and results

The outlined empirical strategy requires data on students' education outcomes, their backgrounds, and the characteristics of the area where they attend school. Data on academic outcomes come from the 2002 SIMCE test that was applied to 4th graders.[16] This test is applied nationwide since 1988 to most than 90% of students of the relevant

[13] Given the historical roots of this variable and the relationship with availability of schools, our proposed instrument might also be capturing historically given differences in the propensity to enroll students in private schools in different areas.

[14] I will focus my discussion on the effects of priests per capita for brevity sake. In general, signs of other covariates are consistent with results in papers analyzing them in detail.

[15] In addition to including violent crime as a measure of security, the motivation for including it is adding a proxy for social capital. I do not have data on traditional measures of social capital, but there seems to be a significant relationship between social capital and violent crime (Lederman et al., 2002).

[16] I will use the 2002 test because it is the only year for which I have individual measures of education outcomes, family background, and private and public expenses in each student education.

grade. I will use the average of the Math and Spanish portions of the test (standardized to have an average of 0 and a standard deviation of 1) as measure of academic outcomes.

Regressions were run also using as dependent variable a measure of productivity. The productivity index is computed as the ratio of test scores and (the log) of total expenses related to each student education, as proposed by Hoxby (2000).[17] Total expenses include both private and (the largest part of) public expenses in each student education. Data on private expenses come from a survey on parents of students taking the SIMCE test and reflect household expenditure in each student education (up to miss-reporting). The per-student subsidy was computed by applying to each school the relevant formulas published by the Ministry of Education (the subsidy depends upon whether the school is located in an urban/rural area, the kind of education it provides, and the level of student co-payment, among other factors). Computing public non-voucher expenditure was more difficult because there are various public expenses, especially for public-owned schools. I include the two major sources of additional expenditure for primary schools: transfers which are not included in the per-student subsidy, and participation in the two most important public programs targeted to complement the per-student subsidy (the P-900 program targeted at the worst performing schools and the MECE program focused in funding pedagogical innovations). Data on non-voucher transfers come from the Chilean Municipal Dataset available at http://www.sinim.cl/ and data on the P-900 and MECE programs come from several publications of the Ministry of Education. Notice, that however the measure of expenditure is imperfect; it accurately includes the majority of expenditure (namely, private expenses and the per-student subsidy).

Data on socioeconomic background of students come from the survey on parents. Students' socioeconomic background is measured using educational attainment of their mothers.[18] I use five categories measuring mothers schooling (having attained at most primary education, secondary general education, secondary technical education, post-secondary technical education, or college or postgraduate education).

[17] Using total expenses (instead of the log of total expenses) has no significant impact on results.

[18] I will focus in mother education because it is the variable more used in the literature and because other variables (such as family income and father education) are highly collinear with mother education. Qualitative results do not change when including those variables or replacing mother schooling with them.

Data on private enrollment are drawn from the Ministry of Education enrollment files. As previously mentioned this variable was measured as the ratio of private enrollment to total enrollment in each school market. Data on priests per person and the share of Catholics in each Chilean diocese come from www.catholic-hierarchy.org.

Observations on 138,858 and 210,187 students are available for estimation purposes for productivity and test scores, respectively. Selected descriptive statistics are shown in Table 3. A couple of comments about these statistics. First, the average student attends a school in a market where private enrollment is about 40%, but there is a lot of variation. Second, the average school market has about 0.16 priests per 1,000 people and the average school market has a share of Catholics close to 75%, with the minimum being close to 55% and the maximum close to 95%. Thus, there is variation in variables related to the identification strategy. Finally, heterogeneity is higher at the class-room and school levels than at the market level, suggesting at least some sorting at disaggregated levels.

Table 4 presents results for an OLS regression of private school enrollment as a function of our suggested instrument and other covariates (i.e. the first stage of equation 1). Catholic priests are significantly related to the share of private enrollment in a market. One standard deviation of the number of priests per person boosts private enrollment by about 8 percentage points. Results regarding other covariates suggest that private enrollment is higher in areas with more educated households and more inequality in pre-school characteristics (as in Hsieh and Urquiola, 2003). Somewhat surprisingly, the share of Catholics presents an insignificant, but *negative*, effect on private enrollment. Traditional explanations argue that the higher the share of Catholics in an area, the higher the demand for Catholic schools (Hoxby, 1994). However, a potential explanation is that there is another effect going in the opposite direction: Catholics living in areas where affiliation to the Church is relatively low are not able to affect the way public schools teach as strongly as in markets where they are the overwhelming mass (e.g. in the latter, parents are almost sure that teachers are Catholic), and therefore need to settle their own schools. Finally, the F-statistic on the excluded instrument is 28.21, suggesting that it is not weakly correlated with C_m.

As initial motivation for our IV-results, Table 5 shows results of reduced form estimates for the model.[19] [20] For both measures of student outcomes, Catholic priests have a significant and positive effect on academic results. Socioeconomic variables at the student level present expected effects: the more educated the mother, the higher the test score and the productivity index. Estimates for peer-group effects suggest that classrooms having students with more educated mothers have better academic outcomes and that the variability of the same variable is insignificant, after controlling for variability at the school and market levels. Finally, the effect of Catholic affiliation is negative for both measures, but marginally insignificant for productivity.[21]

Table 6 presents IV-estimates. Let us begin by analyzing the bottom of the table. OLS effects of private enrollment on student outcomes are negative but insignificant confirming previous results using OLS or a potentially invalid identification strategy (McEwan and Carnoy, 1999; Hsieh and Urquiola, 2003). However, IV estimates are positive and statistically significant. The contrast of OLS and IV estimates by using a Hausman tests (presented at the bottom of the table), suggests that OLS estimates are inconsistent. Second, IV estimates for both measures imply that the effects of inter-school competition are positive and significant. One standard deviation in private enrollment generates about 0.20 standard deviations in test scores and 0.24 standard deviations in productivity. Results for other variables are qualitatively similar than for reduced form estimates.

Results are obtained after controlling for students' socioeconomic background and classroom, school, and market characteristics, so it is highly unlikely that peer-group effects are not controlled for in these regressions. Moreover, results also suggest that effects of more private competition not only affect measures of test scores (output), but

[19] In all results presented I will show only estimates for the effects of competition, Catholic affiliation, and student and classroom socioeconomic variables. I do that for brevity sake and because either the other variables were statistically insignificant or are hard to interpret (mainly because of collinearity among them). As the focus of this research is not on peer-group effects, the other variables are included to improve the fit of the equation and to isolate the effect of choice of peer-group effects (as in Hoxby, 2000).

[20] The units of the productivity index are not intuitive, thus in all regressions for productivity I show in square brackets each coefficient as a share of one standard deviation of the dependent variable.

[21] Different results for the effect of the share of Catholics on tests scores and productivity may reflect the above-mentioned fact that Catholic schools share some costs with other activities or that they receive private transfers which are not included in the productivity index.

also how effectively output is produced. The last point is relevant because it implies that more choice is beneficial for students without requiring higher expenses.

In a further test of the importance of incentives provided by school choice to agents operating schools, it still remains analyzing if the effect of choice depends upon differences in incentives faced by dissimilar schools. As previously discussed, qualitative evidence suggest that public schools operate under a "softer" budget constraint than private schools. This issue is analyzed in detail in Table 7. The first and second rows show estimates of the effects of private enrollment on educational outcomes for students attending public and subsidized private schools, respectively. Interestingly, point estimates suggest that effects of school choice for students attending public schools are significantly lower than for students attending private schools. Actually, more choice does not seem to affect in a significant way test scores and productivity for students in public schools. This evidence brings support to the claim that if competition and choice do not translate into much pressure to teachers and school administrators, schools have a much smaller response to competition.

However, given that students attending subsidized private schools are relatively richer and come from better family backgrounds than students attending public schools (Hsieh and Urquiola, 2003), a potential questioning of previous results is that there are heterogeneous effects of choice (competition) depending on household characteristics. This issue is analyzed in the next four rows of Table 7. In the estimates, I allow the effects of competition to differ for students who come from low-income and low-education and not-low-income and not-low-education families. For the purpose of the analysis, low-income and low-education households are those that live in a household belonging to the lower quarter of the distribution of income and mother education in their education market. Not-low-income and not-low-education households are all others. Notice that households are classified relative to their markets' income and education because the main reason for considering heterogeneous effects of choice is that the effects of competition depends on household differences *within* a school market (Hoxby, 2000). Results provide little evidence of heterogeneous effects of competition; estimates of interaction effects are statistically insignificant both for test scores and productivity.

These results can be explained by schools specializing in different groups of students and suggest that there are benefits of school choice for students in all socioeconomic groups.

To further test if the lack of incentives is relevant for explaining the very small effects of inter-school competition on public schools, the seventh and eighth row of Table 7 presents interaction effects of our measure of competition and an indicator variable for schools having "softer" budget constraints. This indicator variable takes a value of 1 if the student attends a school located in areas with "non-voucher" public transfers located in the highest quarter of the distribution of this variable. Results show that for students attending schools with softer budget constraints, the marginal effect of private enrollment is significantly negative and, even, the total effect is negative and marginally significant (p-value of 0.059 and 0.074 for test scores and productivity, respectively). These results present some evidence for perverse effects of more private competition for those schools faced with more choice *and* very lenient budget constraints, as previously discussed.

For a second time these results might be mistrusted by using the argument that it might be the case that school choice produces stronger productivity effects in richer and more educated municipalities. Indeed, some areas receiving more non-voucher public transfers are also areas having low income and schooling.[22] The last four rows of table 7 analyze this issue. I constructed indicator variables for students living in areas where family income and schooling were located in the lowest quarter of the distribution of each variable. Results of interacting school choice and the indicator variables for poor and less educated areas are insignificant. Thus, this evidence suggests that there are no heterogeneous effects considering now the poorer and less educated *school markets*, and brings additional support to my claim that different incentive schemes is what is driving heterogeneous effects for students attending different schools.

5. Conclusions

Results in this paper shed some light on the effects of expanding school choice and help to derive some general properties that might be helpful for thinking about reforms. First, Chilean students seems to have received a significant benefit of choice in

[22] The correlation of non-voucher transfers per student with per-capita income and average schooling in each municipality is -0.16 and -0.30, respectively.

terms of increased productivity and test scores, controlling for own characteristics, peer-group effects and school market characteristics. Second, the effects are smaller for students attending public schools than for students attending subsidized private schools. This is especially true for students attending schools having *relatively* softer budget constraints. This is very suggestive evidence that school choice need to have financial consequences if it is to produce the expected effects. Third, there is no much support to the claim that students having different social backgrounds benefit in a different manner of more school choice. Finally, these results do not intend to mean that selection or segregation are not relevant issues in the Chilean case, but that controlling for characteristics of students and peers, there are sizeable direct effects of competition.

The last point is crucial for policy implications and proposals of reforms and it is at the heart of recent discussions about the Chilean experience with quasi-vouchers. Various authors have shown that the Chilean system seems to have produced some segregation (e.g. Hsieh and Urquiola, 2003); however, results in this paper imply that letting segregation constant, more choice improves student outcomes for the schools having the right incentives. Thus, if a government wants to correct segregation it is not necessary to damage school choice, but to use the right incentives. For instance, as recent proposals of letting per-student subsidies depend on student characteristics, along the lines of Hoxby (2001); or mandating subsidized schools to have lottery-based allocations of slots when they have excess demand.

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Table 1: Cross-country regressions for private school enrollment

Explanatory Variable	Dependent variable: private school enrollment in the K-12 system in each country[a]		
Priests per capita[b]	0.790 (0.167)	0.857 (0.136)	0.863 (0.144)
Average years of schooling[c]	-0.030 (0.009)	-	-0.016 (0.012)
Per-capita income[d]	-	-0.083 (0.031)	-0.051 (0.040)
Share of Protestants[e]	0.052 (0.002)	0.003 (0.001)	0.003 (0.001)
Share of Catholics[e]	0.002 (0.001)	0.006 (0.002)	0.006 (0.002)
Share of Other Religions[e]	0.003 (0.001)	0.004 (0.001)	0.004 (0.001)
Size of Government[d]	-0.016 (0.006)	-0.017 (0.006)	-0.015 (0.006)
R-square	0.452	0.457	0.457
Number of countries	34	34	34

Notes: Constants not reported. Robust standard errors are in parentheses. Column (1) presents results of a regression including average years of schooling as a measure of development, column (2) incorporates income per-capita as measure of development, and column (3) uses both variables.

Table 2: Municipal level regressions for selected socioeconomic variables

Explanatory Variable	Dependent Variable: Mean Per-Capita Income[f]	Standard Deviation of Per-Capita Income[f]	Mean Infant Mortality[g]	Violent Crime Rate[g]
Priests per person[b]	-0.016 (0.047)	-0.338 (0.607)	-1.456 (1.061)	-0.662 (0.451)
Share of Catholics[b]	0.875 (0.160)	0.458 (0.287)	-0.242 (0.461)	0.153 (0.135)
Mean Schooling[f]	0.244 (0.007)	0.205 (0.013)	-0.127 (0.023)	0.025 (0.013)
Standard Deviation of Schooling[f]	0.137 (0.028)	0.387 (0.058)	0.007 (0.097)	-0.138 (0.071)
R-square	0.878	0.662	0.166	0.053
Number of municipalities	285	285	208	271

Notes: Constants not reported. Robust standard errors are in parentheses.

[a] Source: James (1993).
[b] Source: www.catholic-hierarchy.org .
[c] Source: Barro and Lee (2001). Measured as average years of schooling of the adult population.
[d] Source: *World Development Indicators*, The World Bank. Size of government is measured as the share of government expenditure in total GDP.
[e] Source: *The World Factbook*, CIA. The omitted category is the share of Muslims in total population.
[f] Source: Own calculations based on the survey on parents of students taken the 2002 SIMCE test (see the data section for more details about the survey).
[g] Source: Various household surveys, see http://www.ine.cl/21-regiones/sociales.htm. Violent crime rate is defined as the ratio of reported violent felonies to total population in each municipality.

Table 3: Descriptive Statistics

Variable	Mean	Standard Deviation	Minimum	Maximum
Outcome variables				
Test scores	0.000	1.000	-2.970	2.542
Productivity index	0.010	0.091	-0.285	0.256
Mother schooling				
Highest level of schooling attended is				
Primary	0.341	0.474	0.000	1.000
Secondary, General	0.299	0.458	0.000	1.000
Secondary, Technical	0.148	0.355	0.000	1.000
Post-secondary, Technical	0.093	0.290	0.000	1.000
College or higher	0.119	0.324	0.000	1.000
Total Years of Schooling of the mother	9.993	3.823	1.000	26.000
School market variables related to the identification strategy				
Private enrollment	0.416	0.192	0.000	1.000
Share of Catholics in total population	0.752	0.067	0.564	0.951
Priests per 1,000 people	0.159	0.043	0.061	0.232
Measures of heterogeneity of students (using years of schooling of the mother)				
Standard deviation at the:				
Classroom level	3.092	0.785	0.000	13.435
School level	3.015	0.597	0.000	13.435
Market level	3.582	0.287	1.155	5.066

Table 4. OLS Regressions for Private School Enrollment

Explanatory Variable	Dependent Variable: Share of Private School Enrollment in Total Enrollment at the Market Level
Priests per 1,000 people	1933.967 (364.123)
Share of Catholics in total population	-0.174 (0.134)
Mean of mother schooling	0.049 (0.008)
Standard Deviation of mother schooling	0.056 (0.024)
R-squared	0.353
Number of observations	285
F-test on excluded instrument is 0	28.21

Notes: Constant not reported. Robust standard errors are in parentheses.

Table 5. Reduced Form Regressions for Student Outcomes

	Dependent Variable	
Explanatory Variable	Test scores	Productivity index
Priests per-person	2183.142 (670.543)	243.766 (74.810) [2692.524]
Share of Catholics in total population	-0.294 (0.127)	-0.020 (0.014) [-0.222]
Highest education level attained is primary	-0.557 (0.016)	-0.049 (0.001) [-0.545]
Highest education level attained is secondary general	-0.350 (0.015)	-0.030 (0.001) [-0.328]
Highest education level attained is secondary technical	-0.232 (0.016)	-0.019 (0.001) [-0.209]
Highest education level attained is post-secondary technical	-0.181 (0.015)	-0.015 (0.001) [-0.171]
Mean of years of schooling of mother at classroom level	0.081 (0.008)	0.007 (0.001) [0.082]
Standard deviation of years of schooling of mother at classroom level	0.007 (0.007)	0.000 (0.001) [0.001]
R-squared	0.228	0.229
Sample size (classrooms) [schools] {markets}	210,187 (9304) [5887] {285}	138858 (6555) [4235] {256}

Notes: This table presents estimates for the effects of some variables included in the reduced form of student outcomes (equation 1). Constants are not reported. Standard errors presented in parentheses are computed using the Moulton (1986) correction for grouped data. The numbers in square brackets show each coefficient estimate as a share of a standard deviation of the dependent variable. The omitted category for schooling of mothers is having attained at least college education.

Table 6. IV-Estimates for Student Outcomes

	Dependent Variable	
Explanatory Variable	Test scores	Productivity index
Private enrollment	1.065 (0.511)	0.113 (0.045) [1.251]
Share of Catholics in total population	-0.239 (0.141)	-0.014 (0.015) [-0.160]
Highest education level attained is primary	-0.560 (0.016)	-0.050 (0.001) [-0.547]
Highest education level attained is secondary general	-0.361 (0.019)	-0.031 (0.002) [-0.338]
Highest education level attained is secondary technical	-0.247 (0.021)	-0.020 (0.002) [-0.222]
Highest education level attained is post-secondary technical	-0.189 (0.018)	-0.016 (0.002) [-0.178]
Mean of years of schooling of mother at classroom level	0.081 (0.008)	0.007 (0.001) [0.082]
Standard deviation of years of schooling of mother at classroom level	0.007 (0.007)	0.000 (0.001) {0.000}
OLS-estimate of the effect of private enrollment	-0.182 (0.129)	-0.003 (0.014) [-0.034]
Hauman-test: OLS vs. IV estimates. P-value, null hypothesis is true.	0.012	0.007

Notes: This table presents estimates for the effects of some variables included in equation (1). Same sample size as in table 5. Constants are not reported. Standard errors presented in parentheses are computed using the Moulton (1986) correction for grouped data. The numbers in square brackets show each coefficient estimate as a share of a standard deviation of the dependent variable. The omitted category for schooling of mothers is having attained at least college education.

Table 7. IV-Estimates for Effects of Private Enrollment on Student Outcomes. Additional exercises

	Dependent Variable	
	Test scores	Productivity index
Same specification as in Table 6, students in public schools. Effect of private enrollment	0.241 (0.726)	0.014 (0.103) [0.157]
Same specification as in Table 6, students in subsidized private schools. Effect of private enrollment	1.228 (0.734)	0.148 (0.074) [1.638]
Same specification as in Table 6 and private enrollment interacted with students from low-income households		
Base effect of private enrollment	0.964 (0.489)	0.106 (0.044) [1.169]
Additional effect for students coming from low-income households	0.175 (0.113)	0.019 (0.015) [0.205]
Same specification as in Table 6 and private enrollment interacted with students from low-schooling households		
Base effect of private enrollment	0.870 (0.560)	0.101 (0.050) [1.112]
Additional effect for students coming from low-schooling households	0.198 (0.149)	0.017 (0.017) [0.191]
Same specification as in Table 6 and private enrollment interacted with a dummy for students attending schools having a soft budget constraint		
Base effect of private enrollment	0.870 (0.560)	0.100 (0.032) [1.106]
Additional effect for students attending schools with soft budget constraint	-2.573 (0.758)	-0.238 (0.069) [-2.630]
Same specification as in Table 6 and private enrollment interacted with a dummy for students attending schools located in the poorest areas		
Base effect of private enrollment	1.062 (0.486)	0.113 (0.044) [1.252]
Additional effect for schools located in the poorest areas	-0.467 (0.533)	-0.052 (0.054) [-0.578]
Same specification as in Table 6 and private enrollment interacted with a dummy for students attending schools located in areas with low human capital		
Base effect of private enrollment	0.989 (0.461)	0.104 (0.042) [1.149]
Additional effect for students living in areas with low human capital	-0.263 (0.557)	0.036 (0.067) [0.396]

Notes: This table only presents IV estimates of the effects of private school enrollment on student outcomes when estimating equation (1). When interactions of private enrollment and other variables are included, the interaction of priests per person and the new variable is used as instrument. Constants are not reported. Standard errors presented in parentheses are computed using the Moulton (1986) correction for grouped data. The numbers in square brackets show each coefficient estimate as a share of a standard deviation of the dependent variable.